# Sibal, Lou 1995

## Dr. Lou Sibal Oral History 1995

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Lou Sibal

This is an interview of Dr. Lou Sibal, who played a key role in the Viruses Cancer Program, taken on March 24, 1995. The interviewer is Dr. Carl G. Baker, former Director of the National Cancer Institute

Baker: Lou, first, before we get to the questions I sent you, could you give us a little bit about your background?

Sibal: Well, I got my Bachelor's degree from the University of Illinois at Champaign/Urbana and my Doctoral degree from the University of Colorado in Denver.

Baker: That major? The Ph.D. was in?

Sibal: My Ph.D. was in microbiology. And so I started out as an immunologist, and my first job was back at the University of Illinois Medical School where I taught immunology and microbiology for about a few years before I came to the NIH. For the record, Mary Fink was one of my advisors at the University of Colorado, which is one of the reasons why I came to the NCI. She remembered me. And these are all off the cuff, are they not?

Baker: Yes. Sure.

Sibal: And so that was one of the reasons why I had a connection, let's say, to the NIH. She then, after I was there for a few years, the teaching burden was extremely heavy and I wanted to get back to research, so she told me that the Government was starting a new Special Virus Leukemia Program (SVLP) at that time. This was in 1965. I don't know whether the Program had actually begun at that time, but it was about to start at that time.

Baker: It was already started in '64.

Sibal: And so I came in '65 as a Special Fellow and worked in the Laboratory of Immunology in the Viral Oncology Section. After about a year I became a regular Government employee. And we did the early work, really, in the murine leukemia virus area, eventually working with cat leukemia virus and so on, immunizing primates to determine the different antigenic structure of the various viruses. I think our group was responsible for classifying the viruses from their group-specific and type-specific antigens. So, if we have any claim to fame it was in the immunological breakdown of the virus. And much of that is still talked about. Envelope virus antigens, and core virus antigens are still pretty much talked about today, although they have little key words for them and so on. But that was, I think, our major contribution to that.

Gradually, as the program grew--and I thought it was very successfully growing at that time-- I'm not quite sure, did Mr. Nixon come into it at about that time, or was that a little bit later?

Baker: Who?

Sibal: Nixon.

Baker: Oh, that was later.

Sibal: The "War on Cancer" hadn't yet been started?

Baker: No. A little bit later.

Sibal: The Special Virus Leukemia Program was started. And the planners decided that they would use the contract mechanism and many of us were asked to be Project Officers on contracts which gradually eroded our time in the laboratory and we spent more time managing the contracts that were being negotiated at that particular time in all varieties of virus and immunological--

Baker: Do you feel sorrowful about that?

Sibal: No. I think we moved into it with our eyes wide open. I think the value of that was the fact that we were very closely associated with the laboratory, very much on top of the literature, and therefore we were very good--I'm saying this in general--Project Officers. After a while, after, say, 5-10 years, you lose that intimacy with the laboratory. You don't read as many journals as you did then, and so you're not as good a Project Officer 10 years later.

Baker: Why do you think you read fewer journals?

Sibal: It was just too much--

Baker: Your hands weren't in the lab as much?

Sibal: Yes. Your hands weren't in the laboratory. You didn't have to justify all the different things that you would write about in a paper, for example. It wasn't that kind of intensity. And I think this is a similar thing that happens to the people who work as SRAs (Special Review Administrators) in the Division of Research Grants. They were in the lab, or they were in program, and they get involved in reviews, and there is so much paperwork involved in it that you have less time to look at some of these things and you stray away from the details of the science, even though you always know the overview of it, I think.

Baker: Sure. Okay, let's turn to the questions. The first one is to get your thoughts on who the leading scientists were and what were the key findings in this period from 1950 to 1980, people who come to mind and the key steps and the main findings that set the stage here.

Sibal: Well, first of all, I don't know how far back you want to go, Carl.

Baker: 1950.

Sibal: Oh, only to '50? Okay. You don't want to get to Peyton Rous or anybody like that?

Baker: Well, I picked 1950 because, at that point, nobody believed viruses had anything to do with cancer.

Sibal: Well, I think that Ray Bryan was a big player.

Baker: And so Bryan kept the flame alive. And Joe Beard.

Sibal: Right. Joe Beard. And I'm trying to recall others because Ray Bryan was here, and he was the head, I think, of our group of laboratories.

Baker: For several years he was the only virologist, along with Moloney, who was his technician originally.

Sibal: There was a person that was involved in the mammary tumor virus field.

Baker: Bittner.

Sibal: But Bittner wasn't here.

Baker: He wasn't here. No.

Sibal: Bittner was at Minnesota.

Baker: Well, Andervont was very interested in--

Sibal: Andervont. Howard Andervont. Those two. The two of them really kind of kept the flame going on what might be called cancer viruses at that time. And Ray was very-- I had many dealings with him and I found him to be very stimulating, and I think everybody else was stimulated by him. And at that time Rauscher was, I think, still-- He was just getting out of the laboratory. And John Moloney. And a year or two later Tim O'Conner came. So that was our particular group.But, in more general terms-- I'm trying-- I'm having a hard time recalling a lot of other names of people. You would have to prompt me.

Baker: I think one of the key changes that changed this outlook was the Ludwig Gross finding of cell-free extracts producing leukemia.

Sibal: Right. Ludwig Gross at Brooklyn.

Baker: That was about 1951, but nobody believed him for a couple years. And then Sarah Stewart, of course, in polyoma.

Sibal: I should have mentioned Sarah because I actually worked close by. Our labs were next to Sarah and her polyomavirus work.

Baker: When that work was confirmed it changed the whole outlook and it allowed the viruses area to expand in cancer work.

Sibal: That's right. I thought Gross's contribution, the fact that when you injected a virus into adult animals it didn't reproduce, but when you injected it into young animals it did, and I think, while that sounds very simple today, it was a very key observation to get it to reproduce and so on because, as you know, those viruses disappeared for a while and didn't reappear until later on in the infection.

Do you want to go more in history, or what?

Baker: Yes.

Sibal: A little bit later I think the pupils of Ray Bryan--John Moloney, Dick Rauscher and-- I'm trying to think of some of the other people that made significant contributions. A lot of good things Mary Fink did in the immunological area, where we look back at them as being very simple now, but we didn't know very much about the viruses then and so anything we could learn--

There were other people but I'm having a hard time-- Are you thinking more like at NCI or anywhere?

Baker: Both. NCI and outside.

Sibal: Oh, I remember Lloyd Old at Sloan-Kettering as being very important as far as the immunology of the viruses was concerned. Then there were European people like Otto Mühlbock (I think he died recently), but he was a very strong European supporter of virus research. Some of the British--

Baker: Deinhardt?

Sibal: Fritz Deinhardt? Yes. Fritz was at the University of Northern Iowa when I was there. Yes. He was on the faculty at the same time.

Baker: Then I guess his wife was also in science?

Sibal: Yes. I don't know-- I'm not sure if she was in that area or not. I don't remember.

You know, the Beards, Joe Beard. Now, we're getting a little bit later. Is it okay to move from the '50s?

Baker: Yes. I want to come up to 1980. That's the period.

Sibal: Well, there was Maurice Green at St. Louis who made major-- We're getting now into the hybridization stages of things. And what's our friend in New York? I'm having a--

Baker: Spiegelman?

Sibal: Spiegelman. Sol Spiegelman. We still have at NCI Jeff Sloan, one of his first students.

Baker: And, of course, our friend at Baylor, Joe Melnick.

Sibal: Yes. Melnick. And Mrs. Melnick at that time. I think she's now a psychiatrist, but she was a virologist in those days.

I'm trying to think around the country.

Baker: Did you have anything to do with Chuck Evans?

Sibal: No.

Baker: He was very helpful from Washington state. He was chairman of one of the committees.

Sibal: Some of the European people who were involved, like the Jarretts in Scotland.

Baker: Guy de Thé?

Sibal: I still see Guy de Thé. I've seen him like in the last year or so.

Baker: Bob Stevenson just interviewed him two weeks ago.

Sibal: In Paris, I hope?

Baker: In France. I think it was Paris. Yes.

Sibal: I saw him when he visited John Moloney, and John asked us over for dinner, and we had dinner with him about a year ago now, or so.

Baker: Well, I never knew Graffi, but Graffi was a key figure in settling the leukemia--

Sibal: Svoboda in Czechoslovakia at that time. And I'm trying to think-- Because we did an awful lot of international things. I mentioned the Jarretts in Scotland. And then there was Epstein and the Epstein-Barr virus from England. And Jennifer Harvey, Charlotte Friend, who was very instrumental.

Baker: Well, these are certainly the key ones. Then later, of course, you actually had the oncogene story breaking out, so Baltimore, Temin, Weinberg, Bishop and Varmus were key individuals.

Sibal: Yes. The picture changed a little bit and we got away a little bit away from virology to subvirology into molecular biology.

Baker: It made a real shift in the outlook of the whole field.

Sibal: Yes, as you say, our current NIH Director with Bishop made a major contribution to the field, and so did Temin and Baltimore. And who else? Who were some others? The one person I think we supported in the program--you can help me with his name--but he did the restriction enzyme. He's the one that discovered the restriction enzymes which cut off pieces of DNA.

Baker: Daniel Nathans?

Sibal: Who?

Baker: Nathans.

Sibal: Yes.

Baker: And Arbor in Switzerland was the co-Nobel Prizewinner on that.

Sibal: Yes. He won the Nobel Prize for it. And, as I recall it, although I'm not sure, Nathans was a contractor of ours.

Baker: Well, he was actually on the staff [as] intramural earlier.

Sibal: Oh, you mean he was here at NCI?

Baker: Yes.

Sibal: A lot of the people were originally contractors with us. Many denied it later on. But some got their starts with help from contracts. I recall this. I'm going to tell you this story. A long time ago I went on a site visit to the University of California at San Francisco and we, at that time, were contracting with a former NCI scientists named Leon Leventow, who had two young people working in his lab. One was Harold Varmus and the other was Mike Bishop. And Leventow got a contract from the NCI to work on some virus work there and I believe they were supported under that contract.

Baker: I think they were. Yes. Leventow was in the NCI Lab of Biochemistry when I was. So, he left Greenstein's lab and ended up at San Francisco.

Sibal: He had just recently gone to San Francisco and was setting up his lab and so on. In California too were Peter Vogt who was a major factor and also Duesberg. Vogt made a lot of contributions. And there was another virologist who was Duesberg's mentor before that, and you might remember him.

Baker: Harry Rubin?

Sibal: Harry Rubin was there at Berkeley, but another person at Berkeley, a very good stature as a virologist, not so much as an on--

Baker: Lynette?

Sibal: No. I remember these names, but I was trying to think. He was a virologist at Berkeley. He was probably the head of the department, and Rubin worked for him.

Baker: Of course, Wendell Stanley went to Berkeley.

Sibal: Stanley? Yes. Stanley. Wendell Stanley.

Baker: He was on the National Advisory Cancer Council.

Sibal: Those are people that were very supportive of the Program.

Baker: Stanley, especially since he had a Nobel Prize, was very helpful in going to Congress and supported the request for the extra $10 million dollars that started the Program.

Sibal: Do you keep up with Duesberg's problems?

Baker: Whose problems?

Sibal: Duesberg, you know, claims that HIV doesn't cause AIDS.

Baker: Yes, in *The Scientist*. Do you get this publication?

Sibal: Yes. And he's been written up in a lot of things.

Baker: Yes. He's got another article in there, in the one I just got yesterday, as a matter of fact. He thinks AIDS is due to responses to drugs.

Sibal: Well, I think there are always a lot of cofactors.

Baker: Well, most causation is not "A causes B". It's "A, B, C and D sometimes, in certain proportions, causes X part of the time in some people." So, cause is a very tricky philosophic problem.

Sibal: Some other people also that got early starts are Max Essex, who is doing a lot of work on AIDS now. There is Dani Bolognesi, who is at Duke. And then Intramurally you can't forget Bob Gallo. I consider that while he was in a different division from the Etiology Division, he was always part of us. And some of the investigators that came here during that time, after I did, who made significant contributions: Ed Skolnick, George Todaro, many people that Dr. Huebner interested in coming to this area.

Baker: Did you work much with Huebner?

Sibal: Yes, quite a bit. I did a lot of things with him because he was a big Program person.

Baker: When you first came, he was still not in the Cancer Institute?

Sibal: He was in NIAID then.

Baker: And then he came over a little later.

Sibal: And don't forget Wally Rowe and Janet Hartley. Hartley is still here. Wally Rowe was a very significant person, I think, in the virus area.

Baker: Well, good. I think these are most of the names that come to mind. I think we may have forgotten one or two, but we'll fix that up when we write all this up.

So, to switch from the scientific side to the administrative and management side, who do you think the key persons there were and what kind of decisions did they make that affected the Viruses Cancer Program?

Sibal: Well, first of all, I think your coming and becoming the Director of the Cancer Institute at that time, and your support of the type of planed program that was started at about that time was very instrumental.

Baker: Well, I think it was probably more important before I became Director because it was at that time, using the systems planning we developed, we spelled out the SVLP.

Sibal: You were not the Director when I came. Zubrod was the Director when I came.

Baker: Not of the Institute.

Sibal: Of our division.

Baker: Of the Intramural.

Sibal: Were you then Director of the Institute? I'm not sure.

Baker: No, Ken Endicott was. However, I started working on the Viruses Cancer effort when I was with Dr. Smadel here in this building (Building 1) and NCI started out with $1 million in grants allocated for cancer virology. And then that didn't seem like it was going well enough and so that led to the asking for the Special Virus Leukemia Program with a $10 million dollar special appropriation. Endicott, I would say, made that key decision.

Sibal: Yes. I forgot Endicott. Yes. He was the Director.

Baker: And Bryan, Rauscher, and I, with Zubrod also reviewing, submitted the evidence that justified Endicott asking Shannon whether he go to Congress for this. And Shannon wanted full information, so he asked for additional information, which we supplied, and then he said yes. And Endicott then went and asked for this, and Wendell Stanley was very helpful in testifying in favor of that.

Sibal: Those are things that I--

Baker: You weren't involved in.

Sibal: --that I was not involved in. It was probably a little before my time, or else I was at a place where people weren't sharing that kind of information with me until later on.

Baker: Well, it was at a different level than where you were working. So, I was Associate Director for Program, and that's when Endicott said, "Okay, you fellows have been talking about planning; give me a plan for a $10 million dollar program." And Rauscher and Carrese and I took 3 weeks off to hammer out those plans, which I believe were helpful.

Sibal: Yes. I remember that. I've forgotten the name of the planning system but--

Baker: Convergence Technique was the name we gave it in the paper we wrote.

Sibal: Yes. Which was Carrese's strong involvement. And I remember many a session where we sat around and plotted things like that.

Baker: I think the people in Chemotherapy used those plans more fully than the virologists. In fact, I think a lot of the virologists did not pay any attention to the plans. But, you see, again it's a different level of operation. The systems planning was first really put together to help develop the budget. How do you decide priorities on what funds you're going to ask for and why you're asking for them, how they fit together? And the systems networking technique is useful for that purpose.

Sibal: I know you're also talking about Sol Shephartz and the Chemotherapy Group at that particular time.

Baker: Zubrod, Shephartz, Carrese and I spent 4 weeks developing those Chemotherapy Program plans. And then Saffioti, Carrese, and I developed the Carcinogenesis Program plans later, and so on. But anyway, much of this was all part of the development, I guess, before you got here.

Sibal: Yes. Or else it was going on at the time and I wasn't aware of it. The first out-of-the-laboratory job that I left after that was Rauscher hired me, and he had already known at that time, I think, and I didn't, that he was to become the Director of the SVLP.

Baker: Ray Bryan started out as heading this effort, but he didn't take to the administrative side very well, and Rauscher did take to it pretty well, so Rauscher, of course, became the key program manager at that point.

Sibal: Right. And everybody sort of moved up. I mean Rauscher moved up a notch, and Moloney moved into Rauscher's old job, and so on and so forth, and eventually what happened is I worked in Rauscher's office for maybe about a year or so and then, when that all changed, I sort of dropped back into Moloney's office, so I kept the same type of position.

Baker: When you first came you reported to Mary Fink?

Sibal: Yes. She was the Section Chief at the time. Moloney was the Branch Chief at that time. He had just become the Branch Chief at that time. Rauscher was his boss.

Baker: And then later you went in Rauscher's office. What were your duties primarily?

Sibal: I got involved in administration at that time, in the planning and the contract process and so on and so forth, and that continued for a long time in Moloney's office. We did a lot of scientific program planning and I did participate with you all in some of the Convergence Technique type of planning sessions and so on. And over the years of that, why, as money grew in the Program and we had more scientists, there was a lot of management involved, and we sponsored the Hershey Meetings, the famous Hershey Meetings, at Penn State with considerable help from Fred Rapp. They were probably the world's premier viral oncology meetings at that particular time.

Baker: Yes. One of the things that I don't think is sufficiently appreciated was the value of communications: the drawing together of information, coordinating it, and putting it together, and then disseminating it, part of which were these meetings that became very important.

Sibal: Every year we would gather our contractors, advisors, and staff together and have exchanges of information at that meeting. I think it made the program move ahead faster and so on.

Baker: And I guess Gallo is continuing with an annual meeting of this type that's become very popular.

Sibal: He seemed to like those meetings, so he's duplicated them in downtown Bethesda.

I think, at that time Fred Rapp, who is an excellent virologist, was very supportive of the Program, and it made the planning of the meeting easier because of his help. The meetings were held at Hershey because he was very supportive of it and helped us work out the logistics.

Baker: David Yohn was another veterinarian who was very helpful from time to time whom we haven't mentioned.

Sibal: He also was involved in early virology studies and then he got involved with the Leukemia Society of America, I guess, when he went to Ohio State.

And, of course, Bob Stevenson was involved in it too, was he not, in the early planning stages?

Baker: Yes. He ran the Resources Area, which we'll come to in a moment.

So, just what comes to mind when I ask you what you think the effects of your participation was in this program? What do you think you did that was helpful and important?

Sibal: I think there was a time in my life, after I had come here and worked in the laboratory for a few years and had at least made some scientific contributions at that time, that it was a newer field and it was possible to have read or looked at almost everything that was ever done in that area, which seems to be something you can never do anymore. There was a bunch of literature about which you could say, "I've read most of it and I understand it and so on." And I felt like knowing all of that was very helpful when the Program was being organized and when we developed a further plan.

Baker: You say you might not have had that broad a base of literature if you had been working in the lab as a regular academic scientist, say?

Sibal: No. I would have had that. But I gained it by being a lab scientist. And at that time you could grasp it all because there wasn't all that much of it.

Baker: So you think you can't do that now just because of volume?

Sibal: Yes. I think it's very different now. It was a privilege that we had.

Baker: Even with computers?

Sibal: Right. I mean, it's just so much to assimilate. You can only understand a part of the field. But I think we had a better grasp of it then. And I think being so close to it was very helpful in planning the programs and making them go in the right direction and making faster progress than we would have made if we had just sort of let it happen. That, I think, would be my contribution.

Baker: A good point, I think. Well, you know, there is a philosophic conflict between the grants philosophy and-- I don't like to call it the "Contract Program; it's a program that utilized the contract mechanism of funding. And those are different meanings, because the main difference was to coordinate and integrate various disciplines that were necessary to tackle certain problems in cancer, and in the Grants Area you don't get that kind of integration.

Sibal: Well, people don't understand about contracts. First of all, the Special Virus Leukemia Program or the Special Virus Cancer Program, were NIH-oriented operations. They were run by NIH. These were things we felt, as scientists, should be done. And, if we wanted to do them they had to be done under a contract. By law, if NIH wants something to be done for it, it has to be done under a contract.

Baker: Well, the Intramural area, of course, is the other.

Sibal: No. I'm talking about in Extramural Programs. If we solicit something, you can do it under a Grants Program too, but the contract mechanism was there because contracts were extensions of the Intramural area in many cases.

Baker: Well, again, I didn't look at it as either Intramural or Extramural; I looked at it as a broad area of scientific effort which required various disciplines and resources which were not available, and these were tied together, and the reason you used a contract was that in order to tie them together, you needed control over what was done, and you couldn't allow each and every investigator to go his own way if you're going to have a coordinated program.

Sibal: Exactly.

Baker: And Endicott realized that when he was asked to set up the Chemotherapy Program, and that's why contracts were started.

Sibal: Sure. If you're going to ask people to follow a certain statement of work, they aren't going to do that under a grant. The grant has too much freedom for them to do anything they want under that and approach it their own way. But, if you outline the steps in the approach, usually those have to go under a mechanism such as a contract.

Baker: And that was the reason for going that way.

Sibal: Surely. Exactly.

Baker: And I don't like this idea of "versus." You know? To me they're both important. I was a great defender of the grant system for exploratory research where you don't want centralized decision-making. But, you can't do everything that way.

Sibal: No. I agree. If I hadn't agreed with what you're saying I probably would have gotten out of the Program. I believe that too.

Baker: Well, you probably would have.

Sibal: Yes.

Baker: Well, very good.

Sibal: My regret, my biggest regret, is that it got into a contentious situation; that a lot of people believed that contracts were not getting the same type of peer review that grants were, and that was not true.

Baker: I thought they got better review, but not everybody agreed with that either.

Sibal: But you know that the world was believing that that wasn't true; that grants had better reviews because they were centrally controlled and DRG, and all this-- And, you know, I never agreed--I still don't--I don't agree with that. I think that the contracts got good reviews and that we brought in the top people in the world at that time. And we're doing some of that now because we have MERIT Awards. There have been a lot of mechanism changes in grants since you've left too. I mean, we have these long-term MERIT Awards where you can give a grant for seven years to an investigator who has done distinguished work, and then you can renew it. Well, basically, what we were really doing at that time was allowing people who had really made their mark in the field to go on and move this other field ahead. Without that I don't think we would have made as much progress.

Baker: I proposed this idea of making ten year commitments to distinguished investigators to Shannon when I was in Building 1 back in 1965 or so, but he wouldn't hear of that long a period. But in those days the average duration of a grant was almost seven years, and it quickly dropped to three, and I guess now, except for these--

Sibal: It's four now. Four to five. But then, of course, people get renewals and renewals and there are people who have grants for 27 years.

Baker: You see, I worry about the amount of time and effort being spent on writing proposals and reviewing them. We're taking up an awful lot of a scientist's time doing that, a lot of which is wasted.

Sibal: One of the things that I'm involved with now here is streamlining that. We're doing a lot of streamlining.

Baker: People don't trust people enough.

Sibal: Well, a problem is that with the new regime here at NIH we're in what's called "Reinvention," as you've heard many times already, and one of the things we're doing is streamlining the grants procedure to what we're calling "Just in Time." We're not asking people to propose initially detailed budgets on grant applications.

Baker: Yes. I read something about that.

Sibal: Because the chances of their getting a grant are very small. At best it will be one in five. Why do we want 80 percent of the people to send us a detailed budget when we have no hope to even fund them because there are not enough dollars?

Baker: Well, I wish you luck because, if you keep a focus on quality, I'm all for reinventing, but I'm not sure that's going to happen, but we'll see.

Sibal: The other new approach is the triage method, and that is when you really think that something belongs in the bottom half, why spend hours talking about it?

Baker: Very good. As I say, I wish you all luck. Let's turn now to some of the advisory committees we had earlier.

Sibal: That's going to be hard to remember.

Baker: Yes. I've had trouble remembering this too. But some of the people that come to mind who you thought were very helpful, or otherwise?

Sibal: Well, I remember James Watson was there at times. Whether it was early or later, I don't know.

Baker: Later, I think.

Sibal: And-- Oh, boy, this is going to be hard to remember. I remember Fred Rapp was on an advisory committee at times. That's going to be hard for me. And yet I was very much involved with all these because at that time that was part of my responsibilities was to get these committees together and to get people appointed.

Baker: Do you remember some of the chairmen?

Sibal: Boy, Alzheimer's has set in.

Baker: It's amazing, isn't it, because I've had trouble remembering exactly who were on the committees.

Sibal: I used to be the keeper of the Virus Cancer Program booklet, you know, that was issued every year, and then, when I left--

Baker: Do you have any of those?

Sibal: I left-- Yes, I did, when I was in DCE of NCI, and the person who might know where they are would be in that division. But Dave Howell stayed there when I came to Building 1. Do you remember Dave Howell?

Baker: Howell?

Sibal: He used to work for me and John Moloney, and he was in the Program for a long time. And I think he inherited them from me, and they might still--

Baker: But you don't have any of these things at home?

Sibal: No.

Baker: Which is probably all right, because it's not legal to take some of this stuff home.

Sibal: I only wish I did have some of them.

Baker: Victoria Harden, the NIH Historian, is trying to get together memorabilia and what not. If you have anything that might be useful to her, don't forget to let her know.

Sibal: Yes. I know. I met her and I know that she--

Baker: She needs help.

Sibal: You know, a lot of the people whose names we've thrown out so far were, at one time or another, members of our advisory committee, like Spiegelman.

Baker: Melnick must have been on some of your committees because he was very active on the animal production efforts.

Sibal: I'm trying to think. You know, John Trentin who was at Baylor?

Baker: Yes. He was very active.

Sibal: Yes. And you mentioned Evans and so on. I'm trying to go around the country and think of names of people.

Baker: Maurice Green we've mentioned. He was on some--

Sibal: Green. Yes. A lot of those people were advisors at one time or another, because they were deeply involved in the field.

Baker: Well, we'll look those people up if I can find the proper documents.

Sibal: In preparing for our discussion I looked several virus cancer books, and I looked through some of them for--

Baker: Well, that's part of what I was asking you about, do you still have NCI committee books?

Sibal: Not those books. I mean those general books on virology there on the shelf.

Baker: Oh. I thought you meant some of the big books that we had for committee meetings.

Sibal: No. I wish I had those. They were big volume kinds of things.

Baker: Yes, they have this information we're looking for.

Sibal: Yes. They had the summaries of the Hershey Meetings and the programs of the Hershey Meetings.

Baker: You don't know anything about the archives in this building in the basement, do you?

Sibal: Yes. But I don't know that any of it would be there.

Baker: Well, I think I ought to look anyway.

Sibal: Yes. You should. I can tell you the right person to talk to. His name is Dave Porter, and he's in charge of--

Baker: I believe Victoria Harden said that. Where is he?

Sibal: In the basement of this building. Now, when I first came here we had a sub-basement full of things. They've gotten rid of that, and I don't think there are things here that are more than 10 years old.

Baker: Well, Don Fredrickson, I know, has been writing some history and he's been finding some things there, but I don't know how far back they go.

Sibal: I got involved in some big radiation study, and there is a lot of papers in someplace, like in Lanham, or Landover, or something, where they were taken for storage.

Baker: Well, there is a new U.S. Government Archive Building that's just opened on the University of Maryland Campus in College Park. That's worth looking at.

Sibal: You might ask Dave Porter where things were sent when they get moved.

Baker: Yes. I will. Thank you. Do you recall any lay members or political figures that had influence on the Viruses Cancer area at that time?

Sibal: Well certainly, not particularly virus work, but I remember Nixon supported the "War on Cancer," and we were into that at that particular time as far as political figures is concerned.

Baker: Later.

Sibal: It came later. I mean, it didn't hurt the Program.

Baker: Well, Wendell Stanley, as we've already mentioned, was a key person who was a lay member in the sense that he wasn't in the cancer field. Of course Sidney Farber and Mary Lasker were supportive in general--

Sibal: Of everybody almost. Yes.

Baker: --and so I think they come to mind.

Sibal: I remember the meetings that we had that were run by Morris Pollard and, remember, the pet food company, the birdseed, do you remember? They were very supportive of the Virus Program, and we always had a symposium in New York City once a year. Hartz Mountain.

Baker: Right. Hartz Mountain. He provided funds for the meetings.

Sibal: Of the Virus Program meetings.

Baker: Yes. Those were good meetings.

Sibal: Yes. Excellent meetings. And Pollard, who was at Notre Dame, was the organizer, but he was a virologist too.

Baker: Notre Dame reminds me of the germ-free animal area.

Sibal: Yes, that's right. It does.

Baker: And the fellow's name that developed the germ-free animals.

Sibal: I'm trying to remember his name. But I remember that. Yes.

Baker: That played a part, I think. Reyniers was his name. He moved his operation to Florida after he left Notre Dame.

Sibal: I know Ann Landers and those people were very helpful to the Cancer Institute, but that was later, I think. I don't know that she supported this Program.

Baker: Okay. Let's turn to a different area of--

Sibal: Can I interject a quick question? When you sent me these notes you called it the "Viruses Cancer Program" in there. Everything was "Virus Cancer Program." I mean, are you really after the title of the Program or just the topic of viruses cancer?

Baker: The latter; the broader. Put it in perspective. We're focusing on the Program itself, but it's in the total picture.

Sibal: Remember, it started out as the Special Virus Leukemia Program.

Baker: Yes. And it switched when we found we had a lot of--

Sibal: Other DNA viruses.

Baker: Like the Moloney sarcoma virus and all. That opened up the whole thing, so it became obvious we ought to change the name.

Sibal: Right.

Baker: Well, I want to talk about resources now, because I want to compare what resources were available and how they were produced in the early period compared to later, and the role of the Program in this area.

Sibal: I think that was very important because, without them, we wouldn't have been able to move ahead. First of all, I think the Program contributed significantly to the improvement of tissue culture in general. It helped all scientists greatly.

Baker: That was probably the earliest contribution.

Sibal: Right. Secondly, I think that a lot of the developments in electron microscopy were moved ahead with it. We aren't dependent on electron microscopy any more for virus identification but, at that time, it was crucial.

Baker: That was about the only thing then that you had for searching.

Sibal: You have to remember the times, and that was the way it was. You had to find virus particles, or else nobody believed there was a virus there. Another important item was the continuous flow centrifuge developed by Norman Anderson.

Baker: I interviewed him two weeks ago.

Sibal: Oh, yes? Because I don't think we would have ever gotten enough virus to work with under any other circumstances. The fact that we made a lot of virus and gave a lot of it to the world to use is very important because we provided enough material that was standardized and well characterized, to give good results where the results could come together and be meaningful when people reported on them.

Baker: And you really didn't have that capability beforehand.

Sibal: No. We provide some special resources now here at NIH, but most of it is in programs for animal production. I'm very much involved in that now and I realize the value of it. Again, in the SVCP we had animal breeds of mice, and so on and so forth, that were especially sensitive to cancer development or supported the growth of viruses at that particular time when we still had to use mice to reproduce the virus and so on. Some of the purification methods, I think, that were developed at that time to get all of the host material away from the virus material, to tell us which antigens were which, was very important.

What other areas were there at that time?

Baker: Well, the question of identifying viruses, I guess, one of the programs allowed people to send in virus preparations for characterization and identification, although I gather that wasn't a major program.

Sibal: Right.

Baker: Animal husbandry developmental research was done on production of primates, for example. We didn't really know anything about blood counts and things like that in a lot of the primates until this was done. And then, on tissue culture, we really had not solved the problem of freezing cells and thawing them. Some of the earliest research in the Program supported what would be developmental research on what's the best way to freeze and thaw tissue cultures to get high viability.

Sibal: Right. And then a lot of things like what were the sensitivities of the viruses and so on. And a lot of those things have come in handy now with retroviruses being in the HIV field, because we know now how to kill HIV because we knew how to kill a lot of the other retroviruses, and what's dangerous and what's not.

The other thing is, I think we helped develop a lot of the biohazard procedures and safety materials. There was a lot already, but at least I think we moved it forward a little more quickly.

Baker: You may not even realize that the now standard symbol for biohazards was developed from a contract in the Program in which a variety of images were tested in the field for people recognizing it, knowing what it meant, and remembering.

Sibal: I worked in Building 41, you know, when it opened. That was my last area of work. We had this wonderful biohazard containment building with glove boxes, and so on. I noticed they've recently redone the building and upgraded a lot of it, and we're back into using it for TB and AIDS.

Baker: Well, Endicott wanted that building to be completely flexible including movable walls and, by the time everybody got through--Ray Bryan and others on the planning for their offices--only about a third of the building ended up with the kind of flexibility that Endicott had in mind. So he was always kind of laughing about how, behind his back, the scientists changed the whole concept of the building.

Sibal: Well, you know, there's a lot of things you don't think about. For example, the fact that the viruses did not prove to be as contagious as we first thought; in other words they didn't require Level 4 containment.

Baker: But we didn't know that at the time.

Sibal: Yes. We had to find that out, that we didn't need Level 4 grades to work with them, and so on.

Baker: And so I think you could also say that this laid some foundations for biotechnology as it's blossoming today.

Sibal: Sure. And when you get, now, a little further on in the process of our knowledge of viruses, I think it will be more evident that the Program contributed a lot at the molecular level. First of all, I think it really supported the beginnings of molecular biology, and today that's everything. I mean, the fact that you could hybridize things and pick out little pieces of viral information from cell DNA and so on. I think a significant advance was that it helped to make known that there was an enzyme such as reverse transcriptase that would allow us to understand how an RNA virus, for example, could incorporate genetic information derived from itself into the DNA of the cell and disrupt the cell and so on. I mean that's really basic. That claim will raise an argument. I'm sure the basic scientists who were working under grants at that time will claim that we had nothing to do with that, but I don't think that's true. I think we contributed to it.

Baker: It's hard to allocate credit.

Sibal: I mean we share it. I think we should share some of the credit there.

Baker: Well, we will try in writing this history to give a calm account of all of this, hopefully not too biased one way or the other. But you certainly get differences of opinion on--and they're really philosophic differences--on how you go about conducting research. And to me you need both approaches, not one or the other.

Sibal: Right.

Baker: You had an interesting experience on a trip to Russia. Tell us a little bit about that.

Sibal: Well, actually, from the Cancer Institute we developed a collaboration with both the French and the Russians. We had a collaboration with the person who is now the-- Who is it? I can't think of the name of the person that Gallo has had problems with at the Pasteur Institute.

Baker: Montagnier?

Sibal: Yes. Montagnier. And there were a number of French scientists at that time who were interested in a virus.

Baker: Gyu de Thé was one that comes to mind.

Sibal: Yes, Guy de Thé was one. Absolutely. Most of them were centered in Paris. And we would meet with them and they would exchange ideas.

Baker: The guy at Strasbourg?

Sibal: Yes. I can't remember the name. Well, Francoise Haguenau was helpful in getting the French groups together with us and so on. And, in fact, I think that's how Gallo and Montagnier were put together. I remember attending a couple of French meetings and Gallo came over and probably helped in starting the collaboration with them at that institute. At that time Montagnier wasn't at Institute Pasteur; he was somewhere else. I'm trying to think of the names of the people that were at Pasteur, but I can't. Jean-Paul Levy at L'Hospital Saint Louis, who is still quite active in the field, now is in AIDS work and so on, and he's a senior administrator at CNRS, or one of those French equivalents of the NIH. Then, many times, we made several excursions to Russia, and there we had our relationship primarily with Viktor Stanov, who was a virologist, but not an oncovirologist, and Boris Lapin.

Baker: And Zilber.

Sibal: And Zilber was the premier oncovirologist, and his two sons, who both worked with him at the main Cancer Institute in Moscow. I think we aided them tremendously because, after our collaborations, they built the most magnificent Cancer Institute that the Soviet Union had ever had after that.

Baker: Where was that?

Sibal: In Moscow. They redid the whole thing. They rebuilt the whole institute.

Baker: That must have been after I was there, because I went in '72.

Sibal: Right after that.

Baker: I set up the health agreements that Nixon signed. I represented the cancer field, Ted Cooper the Heart Institute, and Dave Rall was in Environmental Sciences. And I didn't realize they had built a new building there.

Sibal: Yes.

Baker: Well, that might have been helpful then, huh?

Sibal: I certainly think so. And I remember probably the last trip I ever took, which is in the later '70s, they'd made a guest house somewhere associated with the Cancer Institute that was close by and we got to stay there one night.

Baker: Oh, they built well then?

Sibal: I think we promoted collaboration with them.

Baker: What do you think we got out of that, we Americans?

Sibal: We got a little bit of resources, I think, in the case of Boris Lapin, and we had some animals, baboons in particular and some that were helpful. And then, as you remember, Lapin came to work here for a year with his staff and so on. Overall not very much. They were not at the same level of science that we were when we started. I think they got more out of it. I think we helped them move into the 20th Century frankly.

Baker: Chemotherapy was the other area.

Sibal: They had probably more progress in it.

Baker: They had a drug or two that we didn't have. But again, I think we gave more the other way. That would be expected, I guess.

Sibal: I think it helped politically to help to break some of the ice between the countries.

Baker: Yes. I remember we reached agreement so fast on collaborative studies in the cancer field that the State Department people said, "There's something wrong here. They haven't behaved this way before. We don't trust them," because they just had never had such an easy agreement between the U.S. and the U.S.S.R. It was very easy in the cancer field to reach agreements on collaboration. The one area we proposed that they didn't want to do was epidemiology, and I think the reason was probably their data collection was insufficient outside of Moscow and Leningrad.

Sibal: Just to ask you-- I remember the person who was the Head of the Academy of Science, and you will remember his name because you may have dealt with him more--

Baker: We met him.

Sibal: And also the name of the Head of the Cancer Institute was Blokhin?

Baker: Blokhin. Yes.

Sibal: Blokhin. Yes. I remember many times, speaking with him, and I thought he was a very sharp person. And is he still alive?

Baker: Last I heard he was.

Sibal: And Trepeznikov?

Baker: Well, he's the one that came later.

Sibal: He's now the Head of the Cancer Institute.

Baker: Yes. I gather. From what I heard, yes. I met him at Carrese's house. It was interesting. Trepeznikov bawled Carrese out for having such a nice home when people in the world didn't live that way, and he couldn't believe that Government worker could have such a nice house. And it was a house right up the street from where I used to live in Fox Hills. It was a nice house, but nothing special in the U.S. But for Trepeznikov, he thought that was immoral to have such a house.

Sibal: We had a couple parties at our house too. You know, they came back and forth. I guess it was five or six times. And they were always impressed with it. But the fact that we invited them to our homes, because we didn't have Federal money to support parties--okay, you can put that on the record--was what helped us, because both the French and the Russians invited us to their homes. And I'd been to, you know, Zilber's son's home. I'd been invited to dinner there, which I thought was unusual, because usually the Russians entertain at a hotel someplace. And the same as the French, who never invite you to their homes, but they did, and I've been many times to the homes of Jean-Paul Levy and Francoise, and they brought in a lot of people to meet us and so on in that way. So I think it helped in the social aspect of it, bringing minds together, or whatever.

Baker: Yes. This is a little oil for the gears. Okay. A question of whether you had much of a grasp of the relative amount of funding in grants in Viruses Cancer work compared with the funding in the contract-supported area?

Sibal: I remember that we got several increases in the contracting area that were significant at the time, but I never got involved in the grant side of it at all.

Baker: That's not surprising. Okay. It didn't take long to answer that question.

Sibal: No. I'm sorry. Who would you talk to that was on the grants side at that time?

Baker: Harvey Scutter earlier. He was Executive Secretary of the V&R Study Section, which began work--expansion--in viruses cancer the year before the Program was founded. I will be interviewing Bill Walter and Bud Herman later.

Sibal: Nowadays, the whole program--of course there is no Virus Cancer Program--but most of it is in the grants area, and they're out at Executive Boulevard, in that unit, and that's what's left of some of the people we had in those days.

Baker: Well, it's all different now.

Sibal: Yes.

Baker: The next question is perhaps a loaded question on whether the Program laid foundations for molecular biology.

Sibal: I think I hit that for you when we were talking about resources and I said I thought it was very significant in the beginnings of molecular biology, for example, in the case where we were talking about restriction enzymes, and hybridization, and so on. All these things, which are really the basis for doing molecular studies and so on. We contributed significantly to that area.

Baker: It's interesting to me that people aren't aware of the historical developments. A lot of people think cancer research started with the new National Cancer Program and refer to results in patients and human subjects as though there was nothing done before 1972. Well, actually, as you know, results in the clinic usually take 10-15 years of development before you get the results. So I see this, and I think most of us who were there before '72 just saw a continuation of what was going on before and there wasn't any real break except the money got bigger. But, in terms of the research programming, it was not any different except in size.

Sibal: And being rather parochial about it, of course, I can only remember some of the things that we supported, but obviously NIGMS and other institutes provided a lot of molecular biology support too, and I'm ignorant of it because I didn't have time to think about all of it, I guess. NIH in general supported a lot of molecular biology in those days.

Baker: Yes.

Sibal: Not to take away from them. But I have no idea how much we contributed, let's say, in relation to them, but I would think it would be very significant.

Baker: Okay. A broader question now about the perception about science by the public. I'm talking beyond biomedicine now. I'm speaking of science in general. Do you think the public is more knowledgeable now and appreciative of science than they were in 1955, or there is no difference?

Sibal: I can answer that, but I have a lot of other background to give you, not from that Program, but because of my involvement with the animal issue. I can tell you the answer is they're less knowledgeable today and there is no question about it, because science, in schools, is extremely poor and they are not getting the proper background there. And we have a lot of education programs going here at NIH to try to help teachers teach science better and to get people interested in science and understanding science. I have an extremely difficult time to tell people why we need to use animals in research and, when I try to explain complicated types of science, neuroscience for example, which is very, very high right now--a lot of breakthroughs in brain research are coming through--it's extremely difficult to try to break down terms in simple enough language to get people to understand why this piece of work is important. The answer is clearly the general population knows less science today than they knew then. Maybe, relatively speaking, they know a lot of things because a lot of breakthroughs have come, but they're not getting it. We need better.

Baker: Well, I agree with you. I was surprised, when I talked to Zubrod, that he thought they were more knowledgeable today. I don't think so.

Sibal: No. It's absolutely the opposite.

Baker: On the other hand, there are better efforts being made to educate. *The Washington Post*, for example, on Mondays, has some very good science reports.

Sibal: Oh, yes. I agree.

Baker: So, if people are interested enough to read it, they can learn. But I don't think a lot of them are interested in learning. And if you need any data about that, how many people have the vaguest idea of the Second Law of Thermodynamics and the significance of it? And yet, that's a tremendously fundamental concept.

Sibal: It's scary. Some schools have dropped their Physics Departments, for example, in the country recently because if they have a small program, they have only five graduate students, or so, and it's expensive to keep going.

Baker: I've been teaching science to non-science majors at the University of Maryland because I think this is needed.

Sibal: You see, we have a Mini-Med School now at NIH. It's run by our Education Office. And we get Fauci and Varmus and other people to come and give lectures in their fields to the people here and visitors.

Baker: Yes. I saw something about that.

Sibal: Yes. It's a very popular program. It's to explain science to people that don't understand science. They're smart, but they've never learned.

Baker: It's not stupidity.

Sibal: Yes. They haven't been exposed to it.

Baker: I agree with you. I think the problem lies in the schools. Now, the science departments in the universities and colleges don't seem to be very interested in teaching science to those who are not going into science, and then they wonder why the public is not more sympathetic to them.

Sibal: That's true. There should be some kind of course at least to give them a background, like the Mini-Med. You know, then they'll understand the body better, etc.

Baker: So these people I was teaching--it was in University College--so most of them had worked all day long, but they had to have one credit in either science or math, and otherwise I wouldn't have had them for that time. So I started with basic physics and took them on through to behavior and brain function, the genetic code, evolution, and the whole works. You can do it. Although some of my friends said, "How could you teach all that stuff?" Well, I had to back and review physics. I hadn't had physics for 40 years. But I worked the problems and gave them some. But they had a tough time with formulas, but you've got to get across the significance of formulas.

Sibal: Sometime, if I decide to retire, perhaps I'd like to teach. When I retire in a year or two, or something, I'd like to be able to do something like that. Not to make money.

Baker: No, no. They don't pay much.

Sibal: I know I'm afraid that I'm going to get bored at home, and I've been sort of a workaholic and, when you do that, then you don't have many interests around the house. You either have to develop them, or you need more outside interest to continue with.

Baker: Well, I got interested in fractal geometry because of these beautiful colored abstract pictures, Mandelbrod sets. Do you know what they are?

Sibal: No.

Baker: Well, you've seen these swirling things with the beautiful colors and what looks like abstract art but it's produced on computers? Well, it's a graphic plotting of some very important areas of mathematics that just started only a decade and a half ago.

Sibal: We have an all new computer system here and we have the new Windows and all those things in our office, and you can draw like that. You can do abstract art on them.

Baker: Yes. I only had a Commodore 64 at that time. It took me all day and night to draw one picture. But that was fun and I also wrote some music for computers.